THE NEW ANTHROPOMORPHISM

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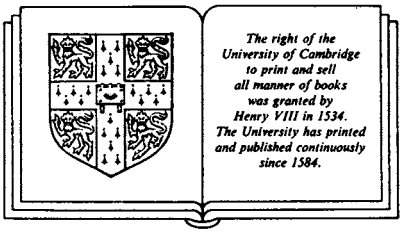

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CHAPTER 1

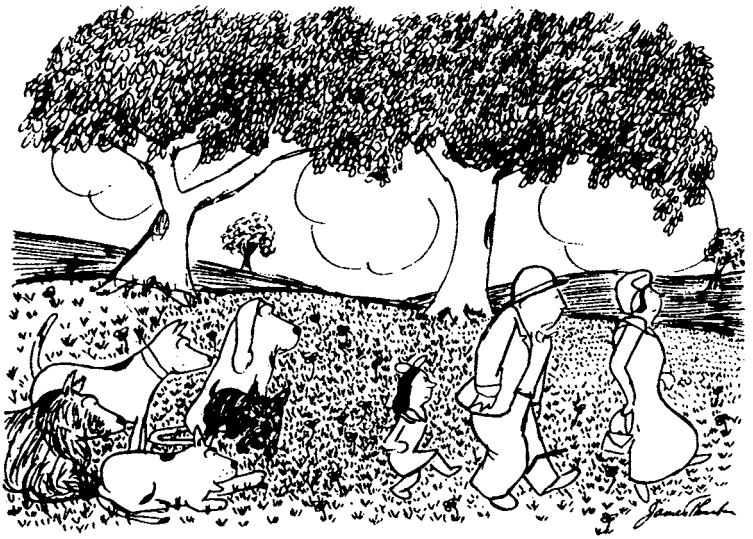
People have always been very ready to believe that animals are like us in having feelings and purposes and acting upon them. Yet there has never been any direct evidence for this ancient anthropomorphic belief, and some three centuries ago René Descartes broke with tradition by arguing that animals were, in principle, machines. Their behaviour, he thought, could be explained straightforwardly by the material mechanisms inside them. Descartes thus sowed the seed of a materialist conception of animal behaviour. The seed fell on rather stony ground and took 200 years to germinate, but by the 1960s the majority of professional students of animal behaviour had rejected traditional anthropomorphism in favour of Descartes on this point. Keeton spelled out their position at that time:

> "Almost all our words have some sort of human connotation, imply some sort of human motivation and purpose. But such motivation and purpose may have no relevance to the behaviour of other animals, and we must constantly guard against unwarranted attribution of human characteristics to other species. Anthropomorphic or teleological thinking has no place in a scientific study of animal behaviour... English (like all human languages), having been developed around human activities and human interpretations, inevitably reflects these, often with a strong cast of supernaturalism.... You are cautioned, therefore, to recognize the pitfalls inherent in any application of human-oriented language to the activities of other animals..." (Keeton 1967, p. 452)

It was a hard grind reaching this point and the first major break with traditional anthropomorphism inevitably went too far. The new approach, championed by Loeb (1900), his pupil Watson (1930) and Skinner (1938), has since come to be called Radical Behaviourism. Putting it crudely, the radical behaviourists more or less discounted internal causes of behaviour, objective as well as subjective. Admittedly this bald description refers only to the later views of Watson and Skinner and is disputed by Skinner's current supporters (e.g. Branch 1982; Lowe 1983; Amsel 1989); but that was the effective message the radical behaviourists left with most workers. Their school dominated the field for the first half of this century but has been very widely rejected over the last few decades. However, the rejection of radical behaviourism does not mean that the majority of workers have gone back to traditional anthropomorphism, although there has been some regression: "The lessons of Behaviourism have not been lost" (M. S. Dawkins 1980). Today the majority are non-radical behaviourists whom I shall call neobehaviourists (see below, p. 6), and they still take anti-anthropomorphism as axiomatic, something mentioned only in passing. For example: "This is ... merely a covert way of adopting an anthropomorphic posture, a posture that we reject when investigating other aspects of behaviour" (McFarland 1989*a*, p. 132). It has again become a matter of serious discussion that human beings as well as animals may be machines. This was a view that La Mettrie, writing a century later than Descartes but much influenced by him, was brave enough to maintain as Descartes himself had not been. Although nowadays, of course, no one is thinking of machines as simple as the ones that they envisaged, nor for that matter of machines that anyone yet knows how to construct (Gray 1987; McGinn 1987; Penrose 1987; Van Gulick 1988; Barlow 1990). Animals as now envisaged are not the stimulus–response automata which anthropomorphists seem to think are the only

alternative that anti-anthropomorphists can offer to animals with minds. "What is needed... is to get rid of the prejudice that machines are essentially simple and deterministic, and to gain an appreciation of the complexity and difficulties in predicting behaviour produced by two or more minds interacting... " (Barlow 1990).

Since it has taken many centuries to achieve the present measure of emancipation from vitalism and anthropomorphism, we may, like Bolles (1975), see this achievement as something to celebrate. It has been a tremendous achievement, something far outweighing the falterings which this book is about. Yet there is no room for complacency. The main point that I want to make is that the scientific study of animal behaviour was inevitably marked from birth by its anthropomorphic parentage and to a significant extent it still is. It has



Empathy without sympathy. (Reproduced with permission. © 1943 James Thurber. © 1971 Helen Thurber and Rosemary A. Thurber. From *Men, women and dogs*, published by Harcourt Brace Jovanovich, Inc.)

had to struggle to free itself from this incubus and the struggle is not over. Anthropomorphism remains much more of a problem than most of today's neobehaviourists believe. But I am calling it *neoanthropomorphism* because the problem has changed somewhat in the last fifty years: anthropomorphism has largely ceased to be explicit and effectively vitalist as it was in the writings of Washburn (1926), Russell (1934, 1946), Bierens de Haan (1937, 1947) and most recently Thorpe (1963, 1965) who was the most cautious: "we can never say that a given piece of behaviour, however elaborate it appears and however much it suggests the presence of consciousness, cannot possibly be the unconscious result of a physiological mechanism.... While, then, we cannot give final proof of consciousness in animals, we can bring evidence to bear which is cumulatively highly impressive and does, I believe, give powerful reasons for concluding that consciousness is a widespread feature of animal life" (Thorpe 1965, p. 474). Accordingly, he took up the explicitly anthropomorphic stance that animal purpose exists at all phylogenetic levels, even the lowest, defining it as "a striving after a future goal retained as some kind of image or idea" (ibid. 1963, p. 3). Likewise Russell: "the objective aim or 'purpose' of the activity controls its detailed course". But the tricky problem now is that neobehaviourists who certainly disapprove in principle of such anthropomorphic thinking sometimes fall victim to it unwittingly. This is not a personal criticism; it was a historical inevitability.

In drawing attention to this danger of the unwitting anthropomorphism that I call neoanthropomorphism I should clear the air straight away by affirming that it is emphatically not my purpose to persuade anyone that anthropomorphic discourse about animal behaviour should be abandoned altogether. This is simply inconceivable for the foreseeable future. Indeed the second main point that I want to make – and it is not

original – is that anthropomorphic thinking about animal behaviour is built into us. We could not abandon it even if we wished to. Besides, we do not wish to. It is dinned into us culturally from earliest childhood. It has presumably also been 'pre-programmed' into our hereditary make-up by natural selection, perhaps because it proved to be useful for predicting and controlling the behaviour of animals. It is therefore useful, incidentally, in scientific research on the adaptiveness of their behaviour (see pp. 88–90).

Yet at the same time our penchant for anthropomorphic interpretations of animal behaviour is a drag on the scientific study of the causal mechanisms of it. There is an inescapable ambiguity and inner conflict in the attitude of students of animal behaviour to anthropomorphism. Their nurture and presumably also their nature prescribe it; their science proscribes it. If the study of animal behaviour is to mature as a science, the process of liberation from the delusions of anthropomorphism must go on. The more so, because what we have been witnessing recently is, on the contrary, less awareness of the dangers, with more indulgence towards and even some resurgence of traditional, explicit anthropomorphism; that bodes ill for this branch of science. Those who would have us go all the way back to traditional explicit anthropomorphism are still a minority but they show us the way things could go if we are not careful. They are not all eccentrics who can be ignored. Moreover they are as full of crusading zeal as the radical behaviourists before them: "We have lived for a very long time with the iniquitous view that it is scientifically disreputable to ascribe feelings and cognitive processes to animals..." (Dunbar 1984*c*). Without going so far, there has been a general drift in that direction (e.g. Cheney & Seyfarth 1990; see p. 91). We are witnessing a new swing of the theoretical pendulum, now back towards anthropomorphism.

This short book addresses particularly, though by no means

only, those students of animal behaviour who are interested in causal mechanisms and are neither anthropomorphists nor radical behaviourists, neither vitalists nor old-fashioned mechanists, but are those whom I am calling neobehaviourists. These are modern behaviourists who differ from their radical forebears in not discounting internal processes in the causation of behaviour (and, often, in not excluding some measure of cognitive activity by their animals if these are 'higher' ones). The grounds for using the term 'neobehaviourist' in this unusual sense are given on pp. 104–5. Most of today's ethologists would come into this category. Ethologists are zoologists by training or adoption, but most of those professional psychologists who qualify themselves as comparative or animal psychologists are also neobehaviourists. Staddon (1989) has recently drawn attention to and deeply deplored the fact that such psychologists are addictively 'anthropocentric', meaning that their aim is to throw light on ***human*** psychology. The inevitable result (now that radical behaviourism has been discredited) is that they are even more susceptible to witting or unwitting anthropomorphism in their approach to animal behaviour than are zoological neobehaviourists. That is a not unimportant theoretical difference between these two groups of people but apart from this the distinction between them and their theories has become more and more blurred since the time when Hinde's (1966) great textbook on animal behaviour came out with its clarion subtitle *A synthesis of ethology and comparative psychology*. The examples that I have chosen to illustrate my theme are therefore drawn from both groups, although I am much less at home with psychology.

The reader may wish to have at least an outline of the zoological neobehaviourist position from which I personally start out and Chapter 2 provides that. The heart of the book is contained in Chapters 3–6 and consists of nineteen essays on ideas which appear to be erroneous and can be traced to

unwitting anthropomorphism. This is not therefore a book with a progressively unfolding theme but rather a collection of essays on important topics in the field, some general and some quite specific. These are extensively interrelated, necessitating frequent cross-references. To start with, Chapter 3 assembles seven concepts that have already been generally recognized as erroneous. Chapters 4–6 deal with twelve further ones on which there is less of a consensus, each chapter dealing with two general and two more specific ideas. Keeton's definition of anthropomorphism (p. 1; cp. Asquith's on p. 9) puts most emphasis on motivation and purpose, but he also gives a stretched definition referring to unspecified "human characteristics" being attributed to other species, an example of which is given, for completeness, in **6.4**. The final chapter considers the constructive steps that we can take to avoid the dangers of anthropomorphism in the study of animal behaviour, while retaining its undoubted advantages.

I would emphasize here that I do not think all the mistakes made by neobehaviourists are traceable to anthropomorphism. Hardly less important as a source of errors is reductionism. Reductionism may of course simply mean looking for the underlying causes of behaviour (see e.g. Horridge 1977; Barlow 1989), an approach which has been spectacularly successful in this century. But the term is being used here in its pejorative sense to mean regarding every whole as no more than the sum of its parts. This is the antithesis of holism (Bonner 1980, pp. 5–8) or "emergentism" as Bunge (1977) called it (see also **6.1**). It assumes that nothing new appears as one moves up from lower to higher integrative levels of a system; and that higher-level events are explicable and predictable entirely in terms of lower-level events. This kind of reductionism has led me into error more than once (e.g. Kennedy 1958). The radical behaviourists were reductionists seeking to reduce all behaviour to simple reflexes and tropisms (the article by Kennedy (1939)

is a dreadful example). Moreover reductionism in animal behaviour *complements* its ostensible opposite, anthropomorphism. Because it cannot account for new, emergent properties it opens the way to semi-mystical explanations as in Jan Smuts's Holist philosophy and, to some extent, Gestalt psychology. Physiologists have a powerful tendency to take a reductionist view of whole-animal behaviour because they habitually think in terms of one bodily function at a time (examples in Kennedy 1972). But this topic would require another book and is hardly touched on here.

Another thing I should say at the outset is that I am of course no exception to my claim that everyone remains in danger of falling into anthropomorphism without noticing (p. 32). I can recall having slipped into anthropomorphism four times at least, taking quite a time to realize each slip. On the first occasion I ascribed the extraordinarily persistent locomotory activity of swarming desert locusts to a "locomotory drive" (Kennedy 1951). That was when ethology had just hit the English-speaking world and recent converts like myself were anxious to acknowledge the existence of internal causes of behaviour which we had been taught to discount. Needless to say my tautology advanced our understanding of the causal mechanisms of locust behaviour not one whit. Nor could it, unfortunately, immunize me against further unconscious lapses into anthropomorphism. It is not long, for example, since I was persuaded for a while by Gallup's (1982) striking claim to have demonstrated self-awareness in chimpanzees, an issue dealt with in **5.3**. All I can do is refer the reader to the quotation from Clark Hull at the end of Chapter 2.